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An
Anomaly in Mathematics

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BY

PHILIP BURTON.

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THE purpose of the following pages is to draw attention to a defect which occurs in works on the Theory of Equations, in some treatises on Algebra and in various other scientific works, and which, owing perhaps to lapse of time, has come to be regarded as no defect at all, but rather as conformable to the "eternal fitness of things." The question whether this defect is to be permanent or removable has hitherto been left an open one; but it is here proposed to offer such arguments, both metaphysical and mathematical, as will establish beyond doubt that the defect would actually be an anomaly quite contrary to and inconsistent with the perfection of abstract science, and that consequently its removal must be feasible by some algebraical process.

The conclusion here stated has been adopted by me since 1904. In that year I wrote out a short statement of the argument for publication; but having at the time an idea that all authorities were against me I did not venture to put it forward. Since then I never had any doubt of the truth of my conclusion, and now offer it for the consideration of the reader, being convinced that in the interest of truth it ought not any longer be withheld.

On account of the length of time during which the question here treated has been left in abeyance and obscurity, the necessity for a treatise of some sort to discuss it will, I think, be admitted by everyone acquainted with the subject; this, then, is my apology for putting forward the present one.

At the same time I am sensible of the defects of the work as regards arrangement and lucidity of argument, and, with a view to recasting it, I was anxious to gain access to some library containing books on the Theory of Equations up to date (several of which, I believe, have been published in recent times); but not having succeeded in this, I have decided to let the tract be printed in its present form.

P. B.

Naas, 24th September, 1910.

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AN ANOMALY IN MATHEMATICS AS DELIVERED IN OUR TEXT-BOOKS.

CHAPTER I.

HOW IS ANOMALY AT ALL POSSIBLE?

I doubt not that many persons upon seeing this tract will be surprised at its title, the appropriateness of which they will consider questionable. They will be likely to say, "It is certain that there can be no anomaly in the science itself, and as for the works in which any of its principles are set forth, since these are constantly under the eyes of experts all over the world, if any error or irregularity should occur in one of them it would be quickly found out and altered, and could not long remain undetected; where then is anomaly at all possible?" This is a view of the subject which will naturally present itself to many readers, and in order to answer the question satisfactorily it may be useful to inquire separately into the propositions which lead up to it.

First then as to the statement that there can be no anomaly in mathematical science, this would seem to follow at once if we regard mathematics as a science of

pure reason, for in such case it must possess essentially such order, regularity, and exactness as will exclude all idea of anomaly or irregularity. This view seems to arise naturally from what may be called the antiquity of the science. "Abstract science," says Sir John Herschel, "is independent of a system of nature—of a creation, of everything, in short, except memory, thought, and reason. Into it the notion of cause does not enter. The truths it is conversant with are necessary ones, and exist independent of cause." (*Discourse on the Study of Natural Philosophy*, p. 15.) Now, mathematics being a part of abstract science, if its principles were true before creation took place and would be true if all bodies in the universe were to vanish into nothing, what remains to be said except that they are dependent on pure reason alone.

Agreeably to all this we find Kant declaring that "all proper mathematical judgments are *a priori* and not empirical because they carry with them necessity which cannot be obtained from experience." (*Prolegomena to any Future Metaphysic*, translated by Dr. Mahaffy, p. 20.) And in another place he says, "Here is a great and established cognition which embraces even now a wonderful sphere and bespeaks hereafter an unbounded extension, which carries with it thoroughly apodictical certainty, that is absolute necessity, which therefore rests on no

empirical grounds and consequently is a pure product of reason and moreover is thoroughly synthetical." (*Idem*, p. 41.)

Such also appears to have been the opinion of Lord Brougham as expressed in the following passage :—"It is certain that the whole science of numbers from the rules of elementary Arithmetic up to the highest branches of the modern calculus could by possibility have been discovered by a person who had never in his life been out of a dark room for the primitive ideas of number might by possibility have suggested themselves to his mind and been made the grounds of all further calculations." (*Discourse of Natural Theology*, note IV., p. 236.)

"It has often been remarked," says Dugald Stewart, "that there is a mutual connection between the different arts and sciences; and that the improvements which are made in one branch of human knowledge, frequently throw light on others, to which it has apparently a very remote relation." (*Philosophy of the Human Mind*, Vol. I., p. 17.) And further on he says :—"In many cases the sciences reflect light on each other; and the general acquisitions which we have made in other pursuits may furnish us with useful helps for the further prosecution of our own." (*Idem*, p. 23.) If this be so

I think it may be expected that the special sciences would tend to illustrate metaphysics, which is more or less general in relation to all of them. Accordingly, in reading works which treat of metaphysical subjects we are constantly meeting with discussions of simple mathematical principles; and the writers are fond of introducing these, not for the purpose of advancing mathematics, but in the interest of the science of which they are treating and for explaining points in connection therewith which are not otherwise easily comprehended by themselves. Thus in Dr. Stewart's work, above quoted, a large part of the second volume is entirely taken up with mathematical disquisitions, and in the remainder of it the words "mathematics" or "mathematical" are to be found in nearly every page; they also occur in many places in the first volume. Kant, too, has been profuse in his application of illustrations from mathematics, at least in the *Prolegomena*, a circumstance which, indeed, is not surprising since he is there found to use language which would seem to imply that he considered mathematics to be a part of metaphysics. For he says, "Hume indeed was prompted (a task worthy of a philosopher) to cast his eye over the whole field of *a priori* cognitions in which the human understanding claims such mighty possessions. But he incautiously severed from it a whole and, indeed, its most valuable province, viz., pure mathe-

matics." (*Prolegomena, etc.*, p. 28.) I do not know whether other metaphysical writers have approved of and adopted this view; so far as I am aware they seem to have avoided the question or left it an open one. Amongst the other metaphysicians, Dr. Reid has availed himself in many instances of mathematical explanations, and Locke is equally fond of introducing them in his speculations; indeed, he has sometimes brought them in when apparently irrelevant to the matter he is discussing.

If mathematics, then, affords a means of advancing metaphysics, is it not to be expected that metaphysical principles would also tend in some manner to advance mathematics. May not arguments be framed and syllogisms "brandished" in such a way as to bring about an improvement in some part of the science or to throw light on some problems which are at present obscure and too difficult for solution? But I think we will search in vain for any such improvement effected by metaphysics. The metaphysicians are all very willing to praise up the science of mathematics, but their speculations do not seem to advance it in any way. Dr. Reid, in stating his views on the science, says, "It has grown from age to age so as to become the loftiest and the most solid fabric that human reason can boast of." And he has thought it well to put forward an argument to prove that the conclusions arrived at in mathematics are all necessary

truths. "I take it," he says, "to be certain that whatever can by just reasoning be inferred from a principle that is necessary must be a necessary truth. Thus, as the axioms in mathematics are all necessary truths, so are all the conclusions drawn from them, that is the whole body of the science." (*Essay on the Intellectual Powers*, p. 647.) But as this principle is taken for granted by other writers it would seem more to the purpose and more useful to mathematics if he had contrived his argument to cover more qualities than the necessity inherent in these truths, which is admitted by all. He might, for instance, have framed, on the lines of Dr. Clarke's or Dr. Cuthbert's famous theological arguments, a syllogism adapted to show the general symmetry and consistency of the formulas resulting from operations in mathematics.

As an example, I think that some syllogism such as the following might be put forward:—Whatsoever hath a necessary existence or any sort of entity independent of cause, whether as subsisting by itself or as an adjunct or attribute, property or quality of another entity must be perfect in its own nature, and immutable; mathematics has such an entity (being admitted above to exist without cause as an attribute of Mind); therefore mathematics must be perfect in its own nature and immutable. This is a syllogism which I think Professor Kant himself would have accepted provided that an appropriate defini-

tion of the term "perfect" could be supplied. The minor premise was constructed by himself, and the major is, I think, more readily allowed than Dr. Clarke's. Moreover, it seems that in the case of Dr. Clarke's argument, all objectors, and notably Dr. Chalmers and Lord Brougham, have passed by the major premise and directed their objections against his minor alone. As to the meaning to be given to the word "perfect," it is not easy to frame an exact definition which may be satisfactory. I do not undertake to do it, but I am sure that no suitable definition could be devised which did not exclude anomaly.

Agreeably to all this does not the experience of everyone who has learned mathematics to any considerable extent convince him, even apart from any general theory on the subject, that it has numerous excellences; that its notation is precise, its definitions strictly accurate, and its axioms untuitively certain; that each successive deduction from the data made by these axioms is rigorously correct, and that the final conclusion is always necessary and certain, being conformable to the eternal fitness of things.

Dr. Dionysius Lardner has illustrated the excellence of one branch of mathematics in the following statement:—
 "The power and facility of investigation which the student obtains by the use of the analytical method are not its only advantages. The great generality of the

theorems, the beautiful symmetry which reigns among the groups of results, the order with which they are developed one from another, offering themselves as unavoidable consequences of the method, and almost independent of the will or the skill of the author, the singular fitness with which the symbolical language of analysis adapts itself so as to represent even to the eye all this order and harmony, are effects too conspicuous not to be immediately noticed.

“Nor is the elegant form which the science thus receives from the hand of analysis a mere object pleasurable to contemplate, but barren of utility. All this order and symmetry which is given as well to the matter as the form, as well to the things expressed as to the characters which express them, not only serves to impress the knowledge indelibly on the memory, but is the fruitful source of further improvement and discovery.” (*Plane and Spherical Trigonometry*; Preface, p. viii.)

For the reasons which have now been advanced and others which might be put forward, it is sufficiently obvious that there can be no anomaly in mathematics, and if in a treatise on any of its branches there occurs an apparent irregularity or deficiency, this must be the fault of the work itself and not of the science. Let us, therefore, consider the second proposition above enunciated, which states that if any error or irregularity should occur

in a mathematical treatise it would be quickly found out and altered, and could not escape detection for any considerable period. Now, if such an assertion were made in connection with any branch of natural science no one, I think, would be found to support it. For the principles of these sciences are so various and intricate and so much bound up with and dependent on the results of observation that there is great room for inaccuracy. No one believes in the infallibility of the human mind, and it is admitted on all hands that the human faculties are not perfect, and require frequent checks, and that therefore the most learned may err as well as the ignorant. "There is," as Sir John Herschel remarks, "no body of knowledge so complete but that it may acquire accession, or so free from error but that it may receive correction in passing through the minds of millions." (*Discourse, etc.*, p. 69.) Even in the refined science of logic the views of some eminent authorities on particular points are various and contradictory, and we find Dugald Stewart asserting of authors whom he named, "all of these writers have in my opinion been occasionally misled in their speculations by a want of attention to the distinction between first principles properly so-called, and the fundamental laws of human belief; . . . nor do I know of any one logician from the time of Aristotle downwards who has entirely avoided this error." (*Philosophy of the Human*

Mind, Vol. ii., p. 95.) In mathematics, of course, there is less liability to error of any kind for its principles are necessarily connected and consistent with the definitions and axioms, and there is always more than one way of exhibiting this connection, whilst in many instances the connection may be shown in a variety of ways, all consistent with one another. Even in mathematical works, however, mistakes which are not printer's errors have actually occurred. Thus Dr. Lardner says that " formulæ expressing relations between the sine and cosine of an arc and those of its multiples were established by Euler and subsequently confirmed by the searching analysis of Lagrange, which have since been proved inaccurate, or true only under particular conditions." (*Trigonometry*, p. 246.) And further on he says:—"In the year 1811 Poisson detected an error in a formula of Euler's expressing the relation between the power of the sine or cosine of an arc and the sines and cosines of certain multiples of the same arc." (*Idem*, p. 246.) Further mistakes of Lagrange were detected by Poinsot in 1825.

But here it may be said that though such mistakes have occurred, they were afterwards rectified, and that in this way the second proposition above alleged holds good. In answer to this I have to remark that there are two classes of mistakes to be considered in reference to this question arising out of the nature of mathematical reasoning. It

is observed by Dugald Stewart that “whereas in all other sciences the propositions which we attempt to establish express facts real or supposed, in mathematics the propositions which we demonstrate only assert a connection between certain suppositions and certain consequences.” Now, problems in Algebra may be considered as of the same nature as propositions in Geometry, for when a problem is solved the result shown thereby is a consequence of the initial supposition, viz., of the data of the problem, and the solution has established a connection between the supposition and the consequence. Should a mistake be made in the process of reasoning or in the evaluation of any of the quantities under consideration, I grant that it is very likely that this would be discovered and rectified before much time had elapsed, and that it is hardly at all possible that in the case of a published work the mistake could escape notice for any considerable time; this may be called a positive error. But there is another kind of error to which the term “negative” may be applicable, and which may be illustrated thus: It has been said above that in mathematics, “the propositions which we demonstrate (and I may add, the problems which we solve) assert a connection between certain suppositions and certain consequences.” A result then of the demonstration of a proposition or of the solution of a problem is that it is shown that the consequence can

always be obtained or deduced from the supposition by reasoning ; but this is not entirely the extent of the connection between them. One might at first sight expect that on reversing the process, and taking the consequence as a datum the supposition could always be deduced directly from it, but this by no means follows. It is true, indeed, that it happens in some cases, viz., those which are in some degree simple and uncomplicated, but in those which are more involved it does not prevail. It is unnecessary here to inquire into the limits of these cases, and I only mention this at all for the purpose of introducing a proposition which I consider to be incontrovertible in relation to the subject. It is this : If we make any supposition and in virtue thereof deduce any consequence ; if we now make a second supposition of a similar kind with the first, but in some respects of a simpler and less complicated nature, and in an analogous manner deduce from this second supposition a corresponding consequence ; then if reversing the process the first supposition can be derived or deduced from the first consequence, the second supposition must also be deducible from the second consequence. For if it is impossible by reasoning to deduce the second supposition from its consequence, there must be an immutable law which bars the solution ; now, this being so, the law which operates thus must *a fortiori* apply also in the first case, which is less simple than the

other, and must prevent the inference of the first supposition from its consequence, which is contrary to hypothesis. Hence if any writer were to include in a mathematical treatise an instance contradicting this plain proposition by reason of his not being able to derive the second supposition from its consequence and should infer from this that such a deduction was impossible he would be putting forward an error or anomaly which, being a negative one, might (if others also failed to remove it) continue for any length of time, even thousands of years, in the absence of a forthcoming solution and explication of the difficulty. Therefore I say that the proposition above alleged, which asserts that anomaly or error could not long prevail undetected, does not hold good, and the possibility of a continuing error or one which exists at the present time is established.

CHAPTER II.

AN ANOMALY LOCATED.

As the simple parts of mathematics, such as geometry and the common algebra, have been diligently studied for thousands of years it would seem impossible that any error or irregularity could now be found in the modern works which treat of these branches. Yet it is in algebra the anomaly occurs which has necessitated the putting forth of this tract; I refer to that defect which in all treatises on equations is described as the “Irreducible case of cubics.” This has been known since 1545, when Cardan, an Italian mathematician, published a treatise under the title *Ars magna sive de Regulis Algebraicis*. One of his problems was the solution of a cubic equation, the method of doing which had not previously been known. It is now called Cardan’s solution, and is said to have been discovered by Nicholas Tartaglea, although it was put forward by Cardan as his own. In this solution it is necessary to take away the second term so that the equation to be solved may be of the form $x^3+ax+b=0$, and after carrying on the requisite operations it is ultimately found that $x = \left(-\frac{b}{2} + \sqrt{\frac{b^2}{4} + \frac{a^3}{27}}\right)^{\frac{1}{3}} + \left(-\frac{b}{2} - \sqrt{\frac{b^2}{4} + \frac{a^3}{27}}\right)^{\frac{1}{3}}$.

Now, when numbers are substituted for the literal coefficients in this result it is found that the formula gives the arithmetical values of the roots in one case, viz., when only one root is real; but when all the roots are real the quantity $\frac{b^2}{4} + \frac{a^3}{27}$ (which we may refer to as the “evolvend”) is always negative, so that its square root cannot be taken, and consequently there is then no arithmetical solution by this method. Seeing then that there is a defect or want of universality in Cardan’s rule, it is reasonable for us to seek a solution by a different method or by some variation of this method, for instance, by assuming the unknown quantity to be equal to the sum or difference of other indeterminate quantities or by assuming different values for the roots and thence forming other equations which can be compared with the given one. This has been done in innumerable ways, and various solutions are thus obtained, but all such are subject to the defect of the “irreducible case.” Solutions may also be obtained from the complete equation or by taking away the third term, etc., but these are also subject to the same defect. Notwithstanding the failure of all such attempts it is plain to me from the foregoing reasoning that a proper solution of the irreducible case must in some way be possible, for otherwise we must allow that it is consistent with the eternal fitness of things that there

should be an anomaly in mathematics. I have held this opinion since 1904, after I had been for some time considering the problem, but a contrary opinion on the matter seems to be now entertained by some writers. The anomaly, then, which I allege is not the failure of Cardan's method to solve all cases of cubics, such failure being quite consistent with and conformable to the strict theory of numbers, but the assertion by scientific writers on Algebra and on Equations that any proper solution of the irreducible case is impossible, or the use by them of language implying such impossibility. But on what grounds are such assertions made? So far as I can see they are made not on any definite principle, but only on the strength of the "argument from experience," which ought to have no force or application in mathematics.

If it were actually true that no solution of the irreducible case is possible there would be a patent anomaly or irregularity before us. For we see that the general equation of the fourth degree can be solved in some of its cases, though it must be more complicated and involved than any equation of the third degree. On the ground then of consistency the simpler equation ought to take precedence of the other in the matter of solvability. If it does not there is an evident irregularity and anomaly. Moreover, this irregularity is aggravated and becomes more obvious by its effect upon the solution of biquadratic

equations. There are several methods of solving these, every one of which requires the solution of a subsidiary cubic equation. In modern treatises on equations various methods of solution are given, such as Descartes', Simpson's, and Euler's, and also a method which may be greatly varied, founded on the principle of symmetrical functions. Now, every one of these is blocked when the roots are real on account of the occurrence of the "irreducible case." Besides the methods now referred to there are various modifications of them which can be employed to effect a solution; these also are blocked in the same manner when the roots are all real. Here then we have anomaly upon anomaly, or rather a succession of anomalies, all arising out of the circumstance that Cardan's method is the only one yet known for the solution of cubic equations in terms of the coefficients, and that it is defective as only applicable to one case of cubics, viz., that in which the equation has only one real root.

Let us now examine the statements of some authorities on the subject and see what light they can throw upon it. The oldest volume which I have at hand is Ree's *Cyclopædia* (published 1819), which treats it as follows:—
 "Irreducible case in Algebra is an expression arising from the solution of certain equations of the third degree which always appears under an imaginary form notwithstanding it is in fact a real quantity, but the reduction of it to a

rational or irrational finite expression has at present resisted the united efforts of many of the most celebrated mathematicians of Europe. Every cubic equation may be reduced to the form $x^3 + ax = b$ and then according to the common rule $x = \left(\frac{b}{2} + \sqrt{\frac{b^2}{4} + \frac{a^3}{27}}\right)^{\frac{1}{3}} \left(\frac{b}{2} - \sqrt{\frac{b^2}{4} + \frac{a^3}{27}}\right)^{\frac{1}{3}}$. Now when $\frac{b^2}{4}$ is less than $\frac{a^3}{27}$ the quantity under the inferior radical, viz., $\sqrt{\frac{b^2}{4} + \frac{a^3}{27}}$ is imaginary, because we cannot extract the square root of a negative quantity; and this is what constitutes that which is generally called the 'irreducible case.' This difficulty soon presented itself to Cardan after Tartalea had communicated to him his method for the solution of cubic equations, which rule is now commonly, though very improperly attributed to the former.

. "Tartalea was himself well aware of the difficulty in question, as appears from some of his private memoranda; and from that time to the present (1819), which is near 300 years, the same impediment remains, notwithstanding the repeated attempts of many very distinguished mathematicians; in fact, there is great reason to suppose, independently of the failure of so many ingenious attempts, that the formula is inexpressible in any other finite form than that under which it naturally arises in the solution.

“ Notwithstanding, however, that no analytical solution can be given to the case in question, every equation of this form has three real roots, which are obtainable by other methods, such as by means of a table of sines and tangents, infinite series, continued fractions, and a new method, lately published by Mr. Barlow in the *Mathematical Repository*, which seems by far the readiest and most accurate of any that has been at present discovered.”
(*Article, Irreducible Case.*)

In a *Treatise on the Theory of Algebraical Equations*, by the Rev. Robert Murphy, published in 1839, after giving the general expressions for the roots of equations of the second, third, and fourth degrees, it is stated that “ These are, moreover, the only forms of irreducible surds by which the general roots of quadratic cubic and biquadratic equations can be respectively expressed ; that which has been called the irreducible case of cubics and which exists in reality only in the arithmetical solution, can, therefore, never be removed by any algebraical solution seeking to express a root by irreducible surds ; we have a sure test of this by the impossibility of making the rational parts of the powers of other surds to coincide with the corresponding quantities deduced from the coefficients of the equation ” (p. 41).

In that inimitable little volume, published in 1844, entitled *An Elementary Treatise on Algebra*, by the late

Professor Thomson, of Glasgow University, Cardan's method is explained, and also its failure to give a solution when all the roots are real, and in reference to the latter case the author remarks:—"This has been called the 'irreducible case,' and it is such that all the attempts made by mathematicians to find any way of computing by means of it the roots of equations in which it occurs have been unsuccessful. Cardan's method, therefore, is of no practical use, except when an equation has only one real root" (p. 285).

This seems a reasonable way of treating the matter, and Professor Thomson was much too cautious to make any assertion in reference to it which the nature of the case did not fully justify.

A beautiful little work entitled *Hymer's Treatise on the Theory of Equations* was published at Cambridge in 1837. It gives an account of Cardan's method, in reference to which it is stated that "this solution only extends to those cases in which the cubic has two impossible roots." And afterwards it is remarked that "In the case of the roots being all real, which for the reason just stated is called the irreducible case, that is when q is negative and $\frac{q^3}{27}$ greater than $\frac{r^2}{4}$ it may be observed that the assumptions in the process $y^3 + z^3 = -r$, $y^3 z^3 = \left(\frac{a}{3}\right)^3$ are inconsistent with one another; for the product of two real quantities

can never exceed the square of half their sum." Does not this satisfactorily show that Cardan's method could not possibly solve any equation of the irreducible case (excepting one having two equal roots), and therefore there ought to be a proper solution of that case entirely distinct and separate from Cardan's. But it did not occur to the author to suggest the possibility of any proper solution.

A text-book much used at the present time is Todhunter's *Treatise on the Theory of Equations*. In this I find the following passage :—" It will now be seen that Cardan's solution of a cubic equation is of little practical use when the roots of the proposed equation are real and unequal. For in this case the expressions for y^3 and z^3 are imaginary ; and although we know that cube roots of these expressions exist, there is no arithmetical method of obtaining them, and no algebraical method of obtaining them exactly. We have the roots in this case exhibited in a form which is algebraically correct, but arithmetically of little value."

" The case in which the three roots of a cubic equation are real and unequal is sometimes called the irreducible case, and sometimes it is said that Cardan's solution fails in this case ; these expressions are used to indicate the fact that the roots are in this case presented to us in a form which is very inconvenient for arithmetical purposes " (p. 98).

From these remarks it is evident that the author was satisfied with Cardan's method as applicable to the irreducible case although it does not furnish an arithmetical solution, and he had no idea that this was inconsistent with the "eternal fitness of things" or that a proper solution was absolutely necessary in view of the perfection and uniformity of mathematical science.

Chambers' Encyclopædia (Edition 1876) has the following on the subject :—"The solution of equations, or, in other words, the evolution of the unknown quantities involved in them, is the chief business of Algebra. But so difficult is this business that except in the simple cases where the unknown quantity rises to no higher than the second degree, all the resources of algebra are as yet inadequate to effect the solution of equations in general and definite terms. For equations of the second degree or quadratic equations, as they are called, there is a rigorous method of solution by a general formula; but as yet no such formula has been discovered for equations even of the third degree. It is true that for equations of the third and fourth degrees, general methods exist, which furnish formulas which express under a finite form the values of the roots. But all such formulas are found to involve imaginary expressions which, except in particular cases, make the actual computations impracticable till the formulas are developed in infinite series, and the

imaginary terms disappear by mutually destroying one another. What is called Cardan's formula, for instance (and all others are reducible to it), is in this predicament whenever the values of the unknown quantity are all real ; and accordingly in nearly all such cases the values are not obtainable from the formula directly, but from the infinite series of which they are the compact expression. But though such formulæ as Cardan's are useless for the purposes of numerical computation, the search for them has led to most of the truths which constitute the general theory of equations, and through which their numerical solutions may be said to have been at last rendered effective and general." (*Article, Equations.*)

The *Edinburgh Encyclopædia*, treating of the subject, has this paragraph :—" Perhaps it may be supposed that although the mode of resolution we have employed fails when q is negative and $\frac{q^3}{27}$ is greater than $\frac{r^3}{4}$ yet there must needs be some other analogous mode of proceeding suited to this particular case. However this may be, it is certain no one has hitherto been able to discover such a method ; so that this case of cubic equations, which has obtained the name of the irreducible case, has given as much trouble to modern mathematicians as the celebrated problems of the trisection of an angle, and

the duplication of the cube gave to the ancients ; and, like these, has in the end baffled all attempts to resolve it." (Edition, 1830, p. 455.)

In the *Encyclopædia Britannica* there is given an account of Cardan's method of solution, after which it is stated that "it even appears that the formulæ which express the roots of cubic equations are not of universal application, for in one case, that is when the roots are all real, they become illusory, so that no conclusion can be drawn from them. The same observation will also apply to the formulæ for the roots of biquadratic equations, because before they can be applied it is always necessary to find the roots of a cubic equation." (Ninth Edition, p. 550.)

In another part of the same work it is stated that "the difficulty attending the irreducible case arises from a real distinction between the two subordinate classes of cubic equations and is insurmountable by the ordinary operations of algebra," and further on we find the statement that the problem in question "is beyond the power of the common algebra." These are very explicit pronouncements in contradiction to the views I have before expressed, and which I have endeavoured to support by reasoning *a priori* ; but as they do not appear to have been made on any sufficient general grounds and seem to be

entirely dependent on the argument from experience, which is foreign to mathematics and ought to have no weight in estimating its principles, I attach no importance to them. If they amounted to this proposition only—“A cubic equation being given whose roots are all real, the calculation of any of those roots after the manner of Cardan’s solution is beyond the power of the common algebra except in a few particular cases,” I could readily assent to it since it is capable of being proved analytically; but if it were stated without the limitation contained in the words “after the manner of Cardan’s solution” I would have no hesitation in declaring the pronouncement to be unscientific and unphilosophical.

CHAPTER III.

CONCLUSION.

From an examination of the works above mentioned and of various other scientific treatises, it would seem that no one who has discussed the subject has so far agreed with my opinion as to insist (and that upon *a priori* grounds) that there must be a proper solution of the irreducible case. Perhaps, however, the silence of writers on this point ought not to be taken as proving them to be of an opinion entirely contrary to that above advocated, and I verily believe that the great majority of writers treating of the question were more inclined to leave it an open one, as they considered that there was not sufficient evidence to decide the matter.

Those writers who maintain that no solution of the irreducible case is possible seem to put stress on a remark made by Bombelli in the sixteenth century to the effect that the problem of the irreducible case corresponded to the ancient problem of the trisection of an angle. The remark itself is true, but when it is attempted to infer by analogy that because it is impossible to solve the latter problem by plane geometry the construction of two parabolas being required for the purpose, it must also be impossible to solve the former by the "common" algebra, the analogy fails completely. To show this we have only to take that other ancient problem, "to double

a cube." This is impossible by plane geometry, and to effect it requires the construction of two parabolas or of a parabola and hyperbola, but the corresponding problem in algebra is "to find two mean proportionals between two given magnitudes," and this is solved with the utmost facility by algebra (see Thomson's *Euclid*, Preface, p. vii.), so that if there be any force at all in the analogy it is greatly in favour of the solvibility of the cubic. I insist, therefore, that there must be a proper solution of the irreducible case distinct from Cardan's method or any modification thereof.

But really it is not difficult to come to a satisfactory conclusion respecting the solvibility or non-solvibility of the "irreducible case" if we consider the matter reasonably. There is a certain principle or proposition which I consider to be self-evident, and which was constantly before me after I had been for some time investigating the problem from the metaphysical point of view. It may be stated thus;—"If there be two equations of the same degree, one of them being simpler or less complicated than the other, and if a solution of the more complicated equation be forthcoming, then the simpler equation must have a simpler and easier solution than the other." This I consider to be as self-evident as Euclid I., 18, which says, "If two sides of a triangle be unequal, the greater side has the greater angle opposite to it"; a principle which also obtains in spherical triangles, and is more obvious

than the homœopathic maxim; *similia similibus curantur*. Now, an equation in the irreducible case must be simpler and less complicated than an equation in Cardan's case, the first member of the latter being the product of factors, some of which involve imaginary or irrational quantities, whilst in the "irreducible" case the factors are all real and less complicated. It is certain, therefore, that the irreducible case, instead of having no solution at all, must be solvable by an easier method than that employed by Cardan for the other case. In fact the reasons bearing on the possibility of a solution are so cogent that it may be said in Shakespearian phrase, "we burn daylight" in investigating them.

It may be pointed out as a remarkable fact that for more than 300 years writers on algebra have been satisfied with a solution of only one case of cubics, and have glossed over the gap or defect arising from the absence of a proper solution of the irreducible case, which they seem to have regarded as consistent with the eternal fitness of things, notwithstanding that a slight consideration of the subject from a metaphysical point of view would expose its inconsistency. This, I think, well illustrates the error of those who in abstract science will depend on views gathered from experience whilst ignoring the results of eternal and *a priori* principles equally at hand for elucidating the general subject and for removing the difficulties incidental thereto.

APPENDIX.

NOTE A.

AN OBJECTION ANSWERED.

I claim that I have in the preceding pages satisfactorily established the proposition that a proper solution of the “irreducible case” is and must be possible, and I shall be pleased to meet any objection which may be brought against it, provided that such objection be made *bonâ fidé* and in the interests of truth, convinced as I am that the investigation of such would tend to throw additional light on the subject and to confirm my reasoning. Someone, however, may think it well to put forward a vague and general objection by saying that I have only stated what was my own opinion, and that this is opposed to the unanimous opinions of all mathematicians since the time of Cardan. Now, it may be as well to point out that such an objection would be entirely unfounded. It is true, indeed, that I have put forward my own opinion, which, however, is not an arbitrary one, nor an opinion taken up at random, but is grounded on incontestible principles, and it is therefore one which I am forced to believe

as true. And it would be quite untrue to say that this opinion is contradicted by all mathematicians since 1545. I have a due respect for that ancient maxim, *Securus judicat orbis terrarum*, which I find quoted even by Professor Huxley, and I admit that it would not be pleasing to me if I were to know that all mathematicians were against me. But so far is this from being the case, I am persuaded that since the time Cardan's solution was published there never was a majority of mathematicians who maintained that no proper solution of the irreducible case was possible.

Let us consider the opinions of the authorities whom I have quoted in previous pages—eight in number. Of these the *Encyclopædia Britannica* is the only one which states absolutely that the problem is insoluble, and “beyond the power of the common algebra.” *Ree's Cyclopædia* only says that “there is great reason to suppose that the formula is inexpressible in any other finite form than that under which it naturally rises in the solution,” but it does not pronounce this opinion absolutely. The writer in the *Edinburgh Encyclopædia* does not seem to have made up his mind whether he would pronounce the solution possible or impossible, for though he states that “perhaps it may be supposed that there must needs be some other analogous mode of proceeding adapted to this particular case,” thus showing that he felt

as if a slur or imperfection was alleged against the science through this fault in the process of solution, yet he adds a statement which implies that the solution would never be found. All the other writers whom I have quoted seem to be in the same predicament, leaving it an open question whether any solution is possible or not. Concerning the opinions of a great number of modern mathematicians it is important to note what the *Edinburgh Encyclopædia* has asserted, viz., that “ this case of cubic equations has given as much trouble to them as the celebrated problems of the trisection of an angle and the duplication of the cube gave to the ancients,” and it is certain that if they had considered the solution hopeless or impossible they would not have continued to take trouble investigating it. If this statement be admitted, no further proof is wanted for the assertion that no majority of mathematicians ever held that a proper solution of the irreducible case is impossible.

NOTE B.

ON SOME CIRCUMSTANCES WHICH HINDERED A DUE ELUCIDATION OF THE PROBLEM.

Various causes seem to have tended to obscure the cubic and to hinder its solution.

(1.) Amongst these may be mentioned, in the first place, its obvious and inherent difficulty, the best evidence of which is the fact that no solution has been put forward after more than three and a half centuries have elapsed since the defect was first perceived. This difficulty is further emphasised in the following extract :—

“ Much labour and thought have been bestowed in order to free the formulæ for the roots of cubic equations from the imaginary expressions that render them unfit for arithmetical computation. In particular instances the difficulty disappears; namely, when the radical quantities are perfect cubes, in which cases the impossible parts of the cube roots destroy one another, so as to leave none but real quantities in the expressions of the roots of the equation. And by expanding the radical quantities we may in all cases obtain the roots of a cubic equation in series of an infinite number of terms free from the imaginary sign. But when it is required to transform the

formulæ for the case of a cubic equation with three real roots, into finite expressions free from impossible quantities and to do so without employing any other than the received notations of algebra, all attempts to solve the problem have led to equations in the same circumstances with the one proposed, and have ended in bringing back the same difficulty; insomuch that equations of the description mentioned are said to be in the irreducible case." (*Encyclopædia Britannica*, 8th Edition, Article, Equations, p. 303.)

(2.) Another cause is the authority and prestige of eminent mathematicians who have tackled the problem and failed to elucidate it. If any of these asserted the impossibility of the solution his statement would be followed out by others who had themselves failed in like manner, and who were rather satisfied that they were not outdone even by more eminent men. I have mentioned before the self-evident principle that the simpler equation must have a simpler solution. Now, this proposition so far as I know is not mentioned or referred to by any writer on mathematics, and I might, perhaps, with some show of reason claim it as a new discovery. But it being a self-evident proposition it is plain to me that every mathematician who has investigated the subject must have felt the force of it. It was an unwritten proposition known to everyone who studied the subject.

How then, it may be asked, could anyone with this principle before him pronounce the opinion that the simpler equation so far from having a simpler solution, has in fact no possible solution at all? I admit that there is here a puzzle as regards the judgment of those who have pronounced absolutely against the possibility of a solution, but I think it illustrates the manner in which the human mind can swerve from rigorous principles which it formerly maintained. For it would appear that after Cardan had published his method everyone who took an interest in the subject tried to find a solution of the irreducible case; they thought it obvious that there should be such a solution, and they evidently believed in the self-evident principle above stated. As time rolled on, however, it was seen that all attempts at the problem were unsuccessful. Then writers began to despair of any solution, and this made some of them attach less importance to the principles which had formerly been acknowledged as rigorous and incontestible; they gradually minimised the force of these until other principles prevailed over their better judgment, and in this way the notion of an impossible solution was set up, though, as I have already remarked, it was held by a minority only.

But there is something to be said in favour of those who leave the question open, for although the self-evident proposition above stated seems absolutely to decide the

matter, yet the apparent difficulty of the subject as exemplified in the failure of all attempts at solution weighs heavily in the opposite scale, and when to this is added the authority of those who pronounce a solution to be impossible, it is not surprising that many should prefer to leave the question in doubt and uncertainty and not insist on the plain deduction which can be made from the general principle referred to.

(3.) Another cause which tended to hinder the solution was Bombelli's remark, made in 1572, to the effect that the problem of the irreducible case corresponded to the ancient problem of the trisection of an angle. Since the trisection cannot be accomplished by plane geometry, but requires the construction of two parabolas or other curves, it was argued from this by analogy that the solution of the irreducible case must be impossible by the common algebra. But I have already shown that this conclusion is altogether done away with and totally reversed when we compare the two ancient problems to trisect an angle and to "double a cube" as the algebraical problem corresponding to the latter can be solved with the greatest facility. Nevertheless, up to the present day Bombelli's remark may be seen quoted as if decisive of the question whether the problem is soluble or insoluble.

(4.) A further cause of the continued obscurity of the problem arises from the circumstance that mathemati-

cians in general seem to have little appreciation of metaphysics. Indeed I suspect that many of them condemn the science altogether, and think that no deduction can be made by means of it except what is vague, uncertain, and indefinite. Nevertheless I think it is not for the benefit of mathematics that such views should be entertained. Are there no puzzles in mathematics itself which require elucidation? If there are, I think it is not wise to lose the chance of clearing up such difficulties through limiting the extent of our speculations.

The ancients had a saying that truth lies at the bottom of a well, which, of course, indicates the difficulty always experienced in finding it. "In attempting the analysis of a new problem, though the discoverer will naturally first try those methods which have been successful in preceding cases, he has no means of assuring himself beforehand which will be successful."

"No general rules of analysis can be laid down." (*National Encyclopædia*, Article "Analysis.") It is my belief that we may sometimes arrive at the solution of a problem, as it were, accidentally or by a process peculiar or suitable to the question at issue (but to which we may be led by speculation) after having exhausted all known methods of inquiry. In the present instance, which has subsisted so long, it may be truly said, "Here we have need of all circumspection." Why then should we refuse

to try any method having the remotest chance of throwing a light on the question, or putting us on the right track towards its solution.

I lately came across in the public journals a statement said to be made by a famous scientist of the present day, to the effect that "science is science and metaphysics is metaphysics, and the two can never meet." If by this he means that the two can never agree, I do not agree with him, for I think it probable that such agreement will ensue in the future, and that it is only a matter of further consideration and reflection.

At present the world is moving very fast; everything is done at a rush, and even the scientist is embarrassed with the cares of life and affected, as others are, by its waves and currents, so that few seem to care to investigate truth unless with a prospect that it will bring pecuniary recompense. But further examination and exploration may do a good deal in metaphysics and elucidate points of interest regarding which scientific writers at the present time know little or nothing;

And of the truth herein

This present *subject* made probation.

For if due consideration from the side of metaphysics had been bestowed upon the problem of the cubic it could not have remained so long in the state of obscurity in which it has hitherto lain. And if this mathematical problem

required much examination and attention, it is reasonable to expect that the deep problems of metaphysics itself would require for their elucidation a great deal more consideration, and that without it they would be inappreciable and unexplorable, just as the very distant stars are inappreciable and imperceptible on the photographic plate and will not be developed thereon unless the latter is a long time exposed to their light.

We may perhaps, in a measure, apply to mathematics what Isaac Taylor says in reference to physics :—

“ The two pioneers of physical philosophy are Accident and Hypothesis ; and so it is that science, while professing to care for nothing but what is certain, actually owes the extension of her domain very much to chance and to conjecture. This humiliating fact, if indeed it should be thought of as humiliating, is forcibly felt, and freely acknowledged, during the spring season of any single branch of science ; for then the particular instances are fresh in everyone’s recollection. But afterwards, and when the new truths have acquired firmness and consistency, and when they have settled down into the form of an ascertained system, and when this system exacts submission, instead of asking for patronage, then is it apt to shrink disdainfully from its early helps, to frown upon hypothesis, and to think itself beyond the reach of any further accessions from accident. This feeling and prac-

tice, however, are not to be admitted ; and philosophy, in its ripest state, should still favour the means of its early triumphs, and freely yield itself to every new chance of advancement.” (*Physical Theory of Another Life*, p. 304.)

(5.) In addition to the circumstances already mentioned there is another which I think has had some effect in obscuring the question ; it is a theory which has been advanced relating to the nature of mathematics itself, and is thus referred to by Dr. Stewart in his *Philosophy of the Human Mind* :—

“ In the opposite extreme to the error which I have now been endeavouring to correct is a paradox which was broached about twenty years ago (that is about 1800) by the late ingenious Dr. Beddoes, and which has since been adopted by some writers whose names are better entitled, on a question of this sort, to give weight to their opinions. By the partisans of this new doctrine it seems to be imagined that—so far from physics being a branch of mathematics—mathematics, and more particularly geometry, is, in reality, only a branch of physics.”

“ The mathematical sciences,” says Dr. Beddoes, “ are sciences of experiment and observation, founded solely on the induction of particular facts ; as much so as mechanics, astronomy, optics, or chemistry. In the kind of evidence there is no difference ; for it originates from perception in

all these cases alike ; but mathematical experiments are more simple and more perfectly within the grasp of our senses, and our perceptions of mathematical objects are clearer."

"A doctrine essentially the same, though expressed in terms not quite so revolting, has been lately sanctioned by Mr. Leslie ; and it is to his view of the argument that I mean to confine my attention at present. "The whole structure of geometry," he remarks, "is grounded on the simple comparison of triangles ; and all the fundamental theorems which relate to this comparison derive their evidence from the mere superposition of the triangles themselves ; a mode of proof which, in reality, is nothing but an ultimate appeal, though of the easiest and most familiar kind to external observation."

Dr. Stewart has ably disproved this theory, and from the conclusion of his reference to it I shall quote the following words :—

"That the reasoning employed by Euclid in proof of the fourth proposition of his first book is completely *demonstrative* will be readily granted by those who compare its different steps with the conclusions to which we were formerly led when treating of the nature of mathematical demonstration. In none of these steps is any appeal made to *facts* resting on the evidence of sense, nor, indeed, to any *facts* whatever. The constant appeal

is to the *definition* of equality. 'Let the triangle ABC,' says Euclid, 'be applied to the triangle DEF; the point A to the point D, and the straight line AB to the straight line DE; the point B *will* coincide with the point E, because AB is *equal* to DE. And AB coinciding with DE, AC *will* coincide with DF, *because the angle* BAC *is equal to the angle* EDF.' A similar remark will be found to apply to every remaining step of the reasoning; and, therefore, this reasoning possesses the peculiar characteristic which distinguishes mathematical evidence from that of all the other sciences—that it rests wholly on hypotheses and definitions, and in no respect upon any statement of facts true or false. The ideas, indeed, of extension, of a triangle, and of equality, presuppose the exercise of our senses; . . . but when we have once acquired the ideas of equality and of a common measure, our mathematical conclusions would not be in the least affected, if all the bodies in the universe should vanish into nothing." (Vol. II., cap. 2.)

After reading this refutation I thought the Beddoestic theory was entirely done away with and would never be heard of again. I was, therefore, greatly surprised at finding by the public journals that it was revived and put forward afresh by the President of the Royal Irish Academy in his address delivered on 9th December, 1907.

From the report of that address given in one of the daily papers I take the following extract :—

“If mathematics were nothing but a process of pure reasoning its claim to be regarded as science might be fairly disputed. It was, however, easily seen in pure geometry that the theory that mathematical processes were merely exercises of pure reasoning was quite erroneous. No amount of pure reasoning could deduce the first theorem in Euclid’s Elements from the axioms without supposing the superposition of one triangle on another. This process was not pure reasoning, but was rather of the nature of an experiment. Every geometrical proof which required a construction was a process of the same kind. The processes of algebra were fundamentally, though not, perhaps, to the same extent as those of geometry, of the nature of experiments. Mathematics in both its great departments was therefore competent to give us new truths. From all they had been considering they could not, he thought, avoid being convinced that mathematics was a most essential part of physical science.” (*Irish Times* Newspaper, December 10, 1907.)

I do not here propose to refute the doctrine of Dr. Beddoes as now revived; Dr. Stewart, I think, has done that sufficiently well, but there was an insinuation made in this address which I think needs a word of comment. It is contained in the sentence, “ Mathematics in both its great departments was therefore competent to give us

new truths," the insinuation being that if it were a science of pure reason it could not give any truths. I ask is not this contrary to common sense, as it is obvious that the laws of nature itself were framed in accordance with mathematical principles, and that all bodies in nature as well as their parts, including the orbs on high and the infinite variety of terrestrial objects, exhibit forms approximating to those established by mathematical definitions? Instead then of mathematics being useless on the supposition stated, common sense would say that a knowledge of it must be essential to a successful study or interpretation of nature.

It will be observed that the theory of Dr. Tarleton differs in no important respect from those of Dr. Beddoes and Professor Leslie above mentioned, though it is delivered in language somewhat more cautious. It is, indeed, a little modified, for we see that an act of the mind (the comparison of imaginary triangles) carried on without any external aid was formerly referred to as an "experiment"; but now it is said to be "rather in the nature of an experiment." It would be interesting to know whether the principles put forward in this address were approved of by many of the audience. There was an objection raised by the Rev. Dr. Bernard, but it was what I may call a half-hearted one, and was not fully insisted on by him. Perhaps the silence of the others

need not be taken as indicative of their views on the question, for I think it has become the practice at such meetings to allow speakers have full scope to employ new hypotheses and extravagances without contradiction lest there should be any hitch in the amenities of public and academic life.

From some statements of Sir John Herschel in his *Discourse on the Study of Natural Philosophy* it would seem that he, too, had a leaning towards the Beddoestic theory.

It seems to me that the theory propounded in this address as well as the theories of Dr. Beddoes and of Professor Leslie are all in unison with that of Hume, and have apparently the same tendency, viz., to detach mathematics from metaphysics, and to make the former a branch of physical science, notwithstanding the fact that the principles of mathematics were true for ages before there were any Physics to unite them to.

As to the theory itself, I consider that it is nothing more than a speculation or hypothesis arising from and fostered by the principles of the utilitarian philosophy which without reason seeks to arrogate to itself the explanation of all things; and I have introduced the subject here only for the purpose of pointing out that the holding of this theory would be likely to have an adverse influence as regards the solution of difficult problems such as that of

the cubic; and if I were to know for certain that such views of the nature of mathematics were generally entertained, it would remove from my mind any surprise which I might feel at finding that the irreducible case had lasted 365 years or for double or treble that period.

Since the delivery of the address referred to I find that papers have been put forward relating to this subject in which it is treated differently. For instance, there is a paper by Mr. R. A. Rogers, F.T.C.D., *On the Logical Basis of Mathematics*, published in *Proceedings of the Royal Irish Academy*, 1908, and another by Mr. Haldane, Secretary-at-War, on the *Logical Foundations of Mathematics* in the January (1909) number of *Mind*. If mathematics be, as alleged, a part of Physics, and if it be founded on the principles of Logic, I suppose it may be right to conclude that Logic also is "an essential part of physical science."

Perhaps I ought to mention here another circumstance which possibly may have had much influence in preventing the solution of the cubic. Whether it really had any influence or not I am not able to say with certainty, but this can be easily judged by those who are cognisant of the principle referred to. What I mean then will be understood from the following extract from the *National Encyclopædia* (Article, *Analysis*):—

"Within the last half century mathematical analysis has made considerable approaches to a state which enables

us to determine, almost immediately, whether a problem can be solved by such means as we possess or not—no small advantage, when it is considered how much time was previously wasted in the attempt to attain impossible results.”

That is to say, mathematicians have got a patent test which being applied to any problem will show at once whether it is solvable or not. Now, I am under this difficulty, that not having access to any recent standard works on mathematics or on the Theory of Equations, I am not able to say what this test is, or whether it has been applied to the “irreducible case.” But supposing that it has been applied to the cubic problem; and that by the principle of this test the problem has been pronounced insoluble during the last half century, anyone can judge that the effect of such a test would be to bar all chance of the solution of the question being arrived at and to convince all who believed in the test that they would only be wasting their time in attempting the solution. If this were so it would have established the probability that the anomaly would continue for ever had we not metaphysics to fall back on as a superior test.

On the other hand, if the test above referred to has been applied to the cubic with the result that the “irreducible case” has been pronounced to be solvable, it is strange that more determined attempts have not been made to reduce it.

NOTE C.

SOME EXPERIENCES OF THE CUBIC.

My knowledge of the cubic problem commenced a long time ago, when I was a pupil of a National School. I had finished the chapter on quadratic equations in Dr. Thomson's *Algebra*, and thence made, through curiosity, an excursion to the end of the book where (in Note B.) was given Cardan's solution of a cubic equation. I followed the steps of the process easily enough, and was satisfied with the reasoning throughout, but I thought the work of the solution rather cumbrous, and it did not then greatly impress me as an interesting problem. In after life I believe that I went over the problem two or three times, but did not consider it very attractive. I did not think that any more could be made out of it than what the book contained.

In the year 1902 I happened to take up the squaring of the circle as an interesting problem. I was led to this through reading an essay of Emerson's in which it was mentioned incidentally. I spent some time occasionally at it and tried it in many different ways, but found that I could not get a result in any way except by that given in the books, viz., by infinite series. As it seems that

there is no limit to the number of such series which may be obtained. I ultimately inferred that no finite number could express the value of each of these, and that consequently the problem is impossible.

Before I had given up this problem I brought with me one day from Dublin a few books purchased off a book-cart in the street. One of these was *Hymer's Treatise on Algebraical Equations*, a beautiful little work, published at Cambridge in 1837. I looked through it at once, and was delighted with its style, which I thought exact, perspicuous, and methodical. I especially fancied the chapters on cubic and biquadratic equations. I read them through and studied them completely, so that I was perfect master of all the principles they contained. But I saw that there was a flaw or inconsistency somewhere, and that this was caused by the irreducible case; it was surely an anomaly that it could not be solved, and I determined to try my hand at it. I took it up in earnest, and after some time it became a fascinating problem. I made many different assumptions with a view of solving it, and thereby obtained hundreds of different cubics, but they were all subject to the irreducible case. I covered quires of paper in these attempts at solution, but to no purpose, and after I had been a good while speculating I was somewhat inclined to believe in the theory which says that no algebraical solution at all is possible. But it was neces-

sary for me to explain the matter in some way in order that I could believe it. The way in which I sought to explain it was this :—According to the proposition before mentioned, that the simpler equation must have a simpler solution there ought to be a solution even easier than Cardan's method; for the “irreducible” case is the simpler of the two, not being involved with impossible or imaginary quantities. But, then, no solution of it was forthcoming. The reason of this, I thought, might be that although the equation in the irreducible case seemed to be simpler than the other, yet from another point of view the other equation might be more perfect, and, therefore, more entitled to a preference in solution. For suppose the equation to arise from a question in arithmetic being proposed which depended on a solution of the cubic. Then the equation which has imaginary roots would seem to give a more definite answer to the question proposed, and thus to be more perfect than an equation in the other case, since the former has only one possible root, whereas if the answer depended on an equation of the irreducible case we would have to make a selection out of the three possible answers.

Another explanation which I attempted was this :—In Cardan's case, if two of the roots be represented by $p + q\sqrt{-1}$ and $p - q\sqrt{-1}$, when these are multiplied together we get $p^2 + q^2$, a real quantity, so that we may then

consider the equation as the product of two rational factors, and it seemed to be easier to separate these two than to solve for three factors all different when the roots are real.

These speculations helped for a while to keep me reconciled to the theory of the text-books, but at any time, I must say, I did not deem them quite satisfactory, and I now wonder that I could ever have considered them to have any value.

And when I began to look at the question from the side of metaphysics, and had read up various metaphysical works, I entirely dissented from this view and came to see that it would be inconsistent with the eternal fitness of things that the simpler case of cubic equations should be without any solution whilst there was actually a solution of equations of the fourth degree. I asked did it not follow *a fortiori* that all equations of the third degree could be resolved? I then investigated the matter anew, and thereby confirmed my conclusion that a proper solution of the irreducible case is absolutely necessary in order to vindicate the dignity of the science, and since 1904 I never had the slightest doubt of the truth of this conclusion.

Being now convinced that I had come upon a correct principle, I prepared for publication in 1904 a statement of my grounds for believing the solution to be possible. I did not, however, deliver this to the printer, for, though

I was thoroughly convinced of the soundness of my argument, I thought that everyone was against me and that I would be arguing *contra mundum*; but I still continued my investigation of the cubic. In 1906 I again had the intention of publishing the statement which I had previously drawn up, but a second time I hesitated to put it forward. On 25th May, 1908, I succeeded in working out a new method of solving the cubic without making use of Cardan's solution. This consisted in first making certain transformations, then completing the cube and extracting the cube root, and thence obtaining the value of a quantity dependent on x .

As I considered the method of "completing the cube" to be a new discovery, and as there was to be a meeting of the British Association in Dublin in the month of August, I determined to send them a paper on the subject. This only occurred to me in the latter end of July, and I had then only a few days to prepare it. I was told that it should be accompanied by an Abstract which was not to contain more than 500 words. I sent my paper to the Secretary of Section A. on 28th July, which, I believe, was the last day for doing so, in order to insure that it would reach in time. But unfortunately in my hurry to send it off I fell into some mistakes which caused it to be returned to me after some days for correction, and it was then too late for the Dublin meeting. But though I

failed to get it brought before the British Association, I was pleased at having sent it on, as it was some satisfaction to think that I had in it sought to maintain with Kant that mathematics is a science of pure reason, and therefore cannot be subject to flaws or defects causing its formulas to be asymmetrical or "illusory," and that it was contrary to the eternal fitness of things that the irreducible case should be without any proper solution. This was the first time I had advanced before any authority the theory which I had framed in 1904, and which I had not then the courage to publish.

I suppose the officials of the British Association who read my paper were surprised or amused at what they, perhaps, considered my simplicity or credulity in seeking to maintain that the Kantian philosophy could have any bearing on the solvibility of a problem in algebra. But, indeed, if we consider the matter impartially there need be no surprise at such a claim; hath not Hamlet truly said:—

"There are more things 'twixt heaven and earth,
Horatio,

Than are dreamt of in your philosophy"—

and, no doubt, the same remark will apply to the utilitarian system. Afterwards I revised my paper, and left out of it any reference to metaphysics or to the irreducible case (which I reserved for the present tract), and

it was published as a pamphlet in 1909 by Sealy, Bryers and Walker, of Dublin, under the title of *A New Method for the Solution of Cubic Equations*.

When sending on my paper to the Secretary of Section A. I also informed him that it was my intention to put forth a paper entitled "An Anomaly in Mathematics"; this is the tract which I now publish, and of which I had projected the outline so long ago as 1904. It will hence appear that the force of authority was influential in keeping back the publication of this paper; and the same principle it is probable may have influenced many others who have endeavoured to solve the problem, and in this way the elucidation of the question has been retarded.

MODIFICATIONS OF THE CUBIC.

Any cubic equation which is proposed for solution may be modified in a variety of ways so as to produce cubics of other forms, and the solution of any of these will lead to the solution of the given equation.

The cubic which was solved by Cardan is $x^3 + ax + b = 0$ and by assuming $x = y + z$, cubing both members and transposing he obtained another cubic equation. From a comparison of these two equations he derived the values of y and z in terms of a and b ; and thence by addition x is found to be =

$$\left(-\frac{b}{2} + \sqrt{\frac{b^2}{4} + \frac{a^3}{27}} \right)^{\frac{1}{3}} + \left(-\frac{b}{2} - \sqrt{\frac{b^2}{4} + \frac{a^3}{27}} \right)^{\frac{1}{3}}.$$

In a similar manner we may solve $x^3 + ax^2 + b = 0$, first assuming $x = y + z + y^{\frac{1}{3}}z^{\frac{1}{3}}$ and it is ultimately found that

$$x = -\frac{a}{3} + \left(-\frac{b}{2} - \frac{a^3}{27} + \sqrt{\frac{b^2}{4} + \frac{a^3b}{27}} \right)^{\frac{1}{3}} + \left(-\frac{b}{2} - \frac{a^3}{27} - \sqrt{\frac{b^2}{4} + \frac{a^3b}{27}} \right)^{\frac{1}{3}}.$$

So also we may solve the complete equation $x^3 + ax^2 + bx + c = 0$ by assuming $x = v + y + z$ and following out the same method we obtain—

$$x = -\frac{a}{3} + \left(\frac{-2a^3 + 9ab - 27c}{54} + \sqrt{\frac{4a^3c - a^2b^2 - 18abc + 4b^3 + 27c^2}{108}} \right)^{\frac{1}{3}} \\ + \left(\frac{-2a^3 + 9ab - 27c}{54} - \sqrt{\frac{4a^3c - a^2b^2 - 18abc + 4b^3 + 27c^2}{108}} \right)^{\frac{1}{3}}.$$

The general cubic $x^3 + ax^2 + bx + c = 0$ may be supposed to be the product of two factors $(x^2 + px + q)$ and $(x + r)$ and by multiplying these together we obtain $x^3 + (p + r)x^2 + (q + pr)x + qr = 0$. From this and the given equation we get $q^3 - bq^2 + acq - c^2 = 0$, and if this can be solved for q , the other quantities, p and r , can be derived from it and thence the values of x can be found.

If the factors be $(px^2 + x + q)$ and $\left(\frac{x}{p} + r\right)$ it would be found that $(ab - c)q^3 - (b^2 + ac)q^2 + 2bcq - c^2 = 0$.

Again suppose the factors to be $(px^2 + pqx + q)$ and $\left(\frac{x}{p} + r\right)$: multiplying these together and eliminating q and r we get $c^2p^3 - 2bc p^2 + (b^2 + ac)p - (ab - c) = 0$ and eliminating p and r we find the equation for q to be $q^3 - 2aq^2 + (a^2 + b)q - (ab - c) = 0$.

In like manner if the factors be assumed to be $\left(px^2 + \frac{q}{r}x + q\right)$ and $\left(\frac{x}{p} + r\right)$, q will be obtained from the equation $q^3 - 2bq^2 + (b^2 + ac)q - (abc - c^2) = 0$.

Also if the factors be $(px^2 + qrx + q)$ and $\left(\frac{x}{p} + r\right)$ we get $(ab - c)q^3 - (b^2c + ac^2)q^2 + 2bc^2q - c^3 = 0$, and if the factors be $(px^2 + rx + q)$ and $\left(\frac{x}{p} + r\right)$ we get $(ab - c)q^3 - (b^2c + ac^2)q^2 + 2bc^2q - c^3 = 0$, so that in this instance q^2 has the same value as q has in the previous one.

If the factors be supposed to be $(x^2 + px + q + r)$ and $(x + q - r)$, q will be obtained from the equation—
 $8q^3 - 4(a+b)q^2 + [2(a+1)(b-c) + 4(a-1)c]q - [c(a-1)^2 + (b-c)^2] = 0$.

The equation may also be supposed to be the difference of two squares, for instance of the squares of the quantities $px^2 + \frac{1}{2p}x + (q+r)$ and $px^2 + (q-r)$. The two squares then are $p^2x^4 + x^3 + \left(\frac{1}{4p^2} + 2pq + 2pr\right)x^2 + \frac{1}{p}(q+r)x + q^2 + 2qr + r^2$ and $p^2x^4 + (2pq - 2pr)x^2 + q^2 - 2qr + r^2$ the difference being $x^3 + \left(\frac{1}{4p^2} + 4pr\right)x^2 + \frac{1}{p}(q+r)x + 4qr$ and when this is compared with the original equation and the quantities p and q are eliminated we obtain $64r^6 - (16ab - 48c)r^4 - (8abc - 46^3 - 12c^2)r^2 - (abc^2 - c^3) = 0$ which is a cubic for r^2 , and from r the other quantities can be deduced, and thence the value of x .

Many other such variations may be shown but the modifications of the cubic which admit of the greatest variety are those which are obtained by assuming the roots to be represented by symmetrical combinations of two or three unknown quantities. Thus suppose the roots to be $(v+y)$, $(v+z)$ and $(y+z)$; then forming the equation which results from these we get $x^3 - 2(v+y+z)x^2 + (v^2 + y^2 + z^2) + 3(vy + vz + yz)x - (v+y+z)(vy + vz + yz) + vyz = 0$ and comparing this with the given equation we find $(v+y+z) = -\frac{a}{2}$;

$(vy + vz + yz) = b - \frac{a^2}{4}$ and $vyz = -\left(\frac{ab}{2} - \frac{a^3}{8} - c\right)$. Hence if the equation $k^3 + \frac{a}{2}k^2 + \left(b - \frac{a^2}{4}\right)k + \left(\frac{ab}{2} - \frac{a^3}{8} - c\right) = 0$ can be solved its roots would be the same as the quantities v , y , and z respectively; and thence the values of $(v + y)$, $(v + z)$ and $(y + z)$ would be found.

Again if the roots be represented by $(v + y - z)$, $(v - y + z)$ and $(-v + y + z)$ the resulting equation would be $x^3 - (v + y + z)x^2 - [(v^2 + y^2 + z^2) - 2(vy + vz + yz)]x + v^3 + y^3 + z^3 - (v + y + z)(vy + vz + yz) + 5vyz = 0$ and we get $(v + y + z) = -a$; $(vy + vz + yz) = \frac{a^2 + b}{4}$ and $vyz = -\left(\frac{ab - c}{8}\right)$ and the equation to be solved will be $k^3 + ak^2 + \left(\frac{a^2 + b}{4}\right)k + \frac{ab - c}{8} = 0$. So also if the roots be assumed to be $(v + y + 2z)$, $(v + 2y + z)$ and $(2v + y + z)$ we get the equation $x^3 - 4(v + y + z)x^2 + [5(v^2 + y^2 + z^2) + 11(vy + vz + yz)]x - 2(v^3 + y^3 + z^3) - 7(v + y + z)(vy + vz + yz) + 5vyz = 0$ and the equation to be solved will be $k^3 + \frac{a}{4}k^2 + \left(b - \frac{5a^2}{16}\right)k - \left(\frac{ab}{4} - \frac{3a^3}{64} - c\right) = 0$. In like manner innumerable different forms of the equation may be produced, for we might take the first of the symmetrical forms of the roots to be $(v + y + 3z)$ or $(v + y + 4z)$ or $(v + y + nz)$ &c., or $(v + y - 3z)$, $(v + y - 4z)$, or $v + y - nz$, &c., and if in any of the resulting equations the value of the absolute coefficient should become $= 0$, the equation would then admit of reduction to a quadratic. Hence it

would seem possible (at least when the coefficients are all integers), to construct a table somewhat similar to Mr. Barlow's, by inspection of which the particular form suited to make the absolute term vanish might be obtained, and by this means the solution of such equations might be effected. But this would not by any means be a proper solution such as we are in quest of; being obtained in a tentative way it would in truth be "rather in the nature of an experiment," and therefore of no use.

A curious modification of the cubic may be shown as follows: Let the equation to be solved be $x^3 + ax^2 + b = 0$ and assume $x = y + z$. Then transposing we have $x - y = z$ and squaring we get $x^2 - 2yx + y^2 = z^2$. Now let the terms of this equation be arranged thus $\frac{1}{2}x^3 - 2yx = (z^2 - y^2) - \frac{1}{2}x^3$ and squaring again and contracting we get $-2yx^3 + 4y^2x^2 = (z^2 - y^2)^2 - (z^2 - y^2)x^2$; and from this by dividing by $-2y$ and transposing, $x^3 - \frac{(4y^2 + (z^2 - y^2))}{2y} + \frac{(z^2 - y^2)^2}{2y} = 0$. Comparing this with the given equation we have $(z^2 - y^2)^2 = 2by$ whence $(z^2 - y^2) = \sqrt{2by}$; also $4y^2 + \sqrt{2by} = -2ay$ and dividing the terms of this equation by \sqrt{y} , $4y^{\frac{3}{2}} + 2ay^{\frac{1}{2}} + \sqrt{2b} = 0$ or squaring both members $16y^3 + 16ay^2 + 4a^2y - 2b = 0$.

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